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Getting at developmental processes through experiments

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Program Dynamics of Healthy Aging

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Abstract

Birren and Bengtson's (1988, p. ix) assessment of the field of aging research that it is "data rich and theory poor" still holds today and applies to developmental science as a whole. The accumulation of descriptive results that are not integrated into overarching theories of development lead to less progress in gaining knowledge about developmental processes than one might wish for, particularly considering its importance for enhancing development across the life span. My wish is that we as developmental researchers become more theory-driven and creative in our empirical approaches to study development. Complementing age-correlative studies, I propose that we make more use of experimental approaches that target the causal mechanisms driving developmental changes (or stability) more directly than by comparing different age groups.

Keywords: Experiments, Brunswikian approach, theory-driven.

If I had one wish... I would want the study of human development make faster progress in understanding developmental *processes* rather than being concerned primarily with age-related differences. In my view, this would require a theory-driven and empirically vigorous approach.

Yes, but this is what developmental science is all about, you might say. We all want this. And probably we all do – if we discount the more petty side of all of academia that is about pushing individual careers and success by increasing the length of one’s publication list rather than actually contributing to accumulating knowledge. Since the attribute “incremental” has become a killer description most certainly preventing a paper to be published, it seems researchers have to come up with a new theory or model (preferably labeled with a catchy acronym) or a description of a – however marginal – phenomenon that is “sexy” and might be more geared towards recounting it at parties rather than to make a lasting (if small) contribution to our area.

But most research *is* incremental, it *should* build on previous work and constitute one step in an upward trend towards more knowledge. I believe that this trend pushing researchers to proclaim their theory or model (instead of working on one that is already out there) and to publish “sexy” research is detrimental to our field as it prevents the more incremental approach, the search for the most appropriate theory and refining it (rather than everybody pushing their own theory), and the building of a cumulative and solid knowledge base that helps understanding human development. In child development, the descriptive approach is often still very prominent. Not that description in and of itself is not informative and necessary as a factual basis on which to build theories. However, as a purely descriptive fact, what does it really tell us if an infant can

do something at 7 or at 11 months of age if this is not embedded in a theoretical framework that would predict one or the other? In adult development and aging, the field suffers more from a predominance of cross-sectional, correlative age comparisons between young and older adults (even though they are often done with extremely sophisticated statistical analyses). Descriptions and correlations are both very limited in contributing in a stringent way to theories about developmental processes.

What may be the remedy? I am not sure how to counteract the pressure to publish more and more (with the consequence to also publish papers that would greatly profit if more work went into them), the push to produce exciting (in the sense of counter-intuitive) results, and the frowning upon incremental research building on somebody else's theory. Changes in this regard would involve a lot of politics at research institutions, universities, and funding agencies. I do believe, however, that we can be more theoretically and empirically vigorous.

Go experimental in developmental research. The problems associated with correlational work in general and with age-correlative, cross-sectional designs in particular, are well documented (e.g., Baltes, Reese, & Nesselroade, 1977) and need no additional summary here. In the current context, I would like to mention only the problem of inferring *causality* and of identifying psychological *processes* resulting in an association between different variables, including age-related differences in such associations. Mediation analyses are generally not well suited to test psychological processes (see Fiedler, Meiser, & Schott, 2011) and extracting age variance in cross-sectional designs in an attempt to test developmental processes is even less appropriate (Hofer & Sliwinski, 2001; Lindenberger, Oertzen, Ghisletta, & Hertzog, 2011). Given

these difficulties in investigating causal mechanisms driving age-related changes and provide insights into *developmental processes*, how can the field move beyond the description of age-related differences or age-differential covariations of different variables?

In a recent paper with Derek Isaacowitz (Freund & Isaacowitz, 2014), we proposed that the field of adult development and aging would profit by using more experimental designs targeting the processes driving age-related differences. There are numerous experiments in this field, but they still mostly use a correlative design regarding age instead of aiming at manipulating the processes that are believed to *cause* age-related differences. Age itself does not represent a psychological mechanism causing changes in cognition, behavior, emotion, or motivation. Instead, age is a carrier variable for psychological processes associated with chronological age that may cause age-related differences (e.g., Wohlwill, 1970). One of the consequences, then, is experimental manipulation needs to target the psychological processes theorized to underlie age-related differences. This requires a well-formulated theory that identifies which age-related processes bring about which age-related differences.

For instance, assume you are interested in age-related differences in speed vs. power cognitive tasks. You might give children, young and old adults the same task either speeded or with a power instruction, and then compare the differences across age groups. Regarding age, this still constitutes a correlative design and, as such, is descriptive. Targeting potential processes *causing* differences between age groups could involve, for instance, experimentally manipulating sensory acuity. This could be done by giving younger adults partially occluded glasses or headphones that filter out certain

frequencies. Such a procedure could simulate sensory functioning in old age (for such an approach attempting to tackle mechanisms driving age-related differences in cognitive functioning see Lindenberger, Scherer, & Baltes, 2001). Another approach could be to use training studies to simulate larger amounts of experience with a certain task or topic if the theory maintains that age-related differences in task performance are due to a lack of experience in one of the age groups (see Park & Reuter-Lorenz, 2009, who make use of this approach to test the scaffolding theory of aging and cognition, STAC).

And when you go experimental, do it in a Brunswikian way. One of the main challenges to good experimentation concerns the external validity of the experiment, that is whether it adequately represents the organism-environment relation (Brunswik, 1952). Based on the assumption that psychological processes evolved as adaptations to the natural environment, Brunswik (1955) posits that psychological processes can only be adequately tested if the experimental stimuli are representative of this natural environment reflecting the person's ecology regarding the frequency, range, value, distribution, and the covariation of its features.

The importance of the person-context transaction for understanding development is also recognized in relational developmental system models (Lerner, 2012; Overton, 2013). Of importance, the transaction between a person and his/her context changes over time. This implies that experimental stimuli that might be representative of the natural environment of one age group might not be representative of the environment of another age group. Thus, when comparing different age groups, experimenters have to ensure the equivalence of stimuli in the ecologies of the different age groups. This clearly requires more work than simply assuming that the same stimulus has the same meaning for

different age groups, but if this point is neglected, it might lead to erroneous conclusions. For instance, when comparing the emotional reactivity between children two and nine years of age, school-related stimuli (e.g., bad grades) likely carry more importance for school children compared to toddlers. Concluding from the use of such stimuli that two-year-olds are not as emotionally reactive as nine-year-olds, however, is not warranted. Although I focus on age in this essay, it is important to note that the same argument applies equally to other individual difference variables that might influence how people interpret experimental stimuli, such as SES, culture, or gender, to name but a few.

The use of representative designs. How to implement a representative design that allows generalizing the results of an experiment to processes occurring in the context in which people actually operate on a day-to-day basis? One way of achieving representativeness according to Brunswik is to sample stimuli in such a way that they are representative of the (group of) persons' ecology. Thus, researchers need to first define a reference class of relevant stimuli (i.e., the class of objects or events that the phenomenon of interest entails) as they occur in the natural environment of the persons under investigation, and then draw a random sample from them. Importantly, this sampling procedure also involves a representation of the *distribution* of the stimuli and the *co-occurrence* of stimuli or different stimulus dimensions.

Non-representative sampling might cause misleading results. In the area of emotion recognition and aging, Isaacowitz and Stanley (2011) argued that the results of many studies might lead to erroneous conclusions because tasks fail to include important contextual cues for emotion recognition in older adults' ecologies (e.g., temporal, interpersonal, environmental cues). The processes involved in emotion recognition used

in non-representative experimental settings might thus be fundamentally different to the ones older adults use in their everyday lives (see also Richter, Dietzel, & Kunzmann, 2010).

Over- or under-sampling certain stimuli or stimulus/feature constellations might also lead to biased results as the typical adaptations that have evolved in the ecology of the person under investigation might not be displayed. For instance, when interested in how teachers react to students of differing abilities, confronting them with a virtual class room of only high or poorly performing students will not elicit the same set of responses from teachers as when confronted with the distribution of students' performances typically encountered in the classrooms of the teachers under investigation. The teachers might react very differently to the *one* poorly performing student in a high performing class than to *one among many* poorly performing students. In the latter case, teachers might spend additional time to go over the materials again because they attribute the poor performance to their own poor instruction. In the other case, the same teachers might feel that the sole poorly performing student should not hold back the rest of the class and ask him/her to go over the materials again by him-/herself.

It would be neither correct to conclude that teachers repeat instructions when students perform poorly nor that they rely on the students' own initiative to catch up. Having poorly performing students catch up by themselves might have been a strategy the teachers came up on the spot in the experimental situation as they have never encountered this stimulus constellation before. Were they confronted with such a situation frequently, they might have developed a very different strategy (e.g., paying more attention during instructions to the one poorly performing student). Note, that

teachers of different kinds of schools (e.g., primary, middle, or high school; public vs. private schools) may encounter different ecologies in their classrooms. Hence, representative sampling also involves identifying different ecologies for different subsamples. This is also true when comparing different age groups that likely differ in their ecologies.

How to identify reference classes. Hammond (1998) proposes to identify reference classes on a theoretical basis and delineate the theoretically relevant structural elements of the stimuli. The stimulus materials (and experimental tasks) then have to mirror these structural elements: "In short, you have to be specific about the variables that a theory tells you are the important ones, namely, the ones that if ignored would produce a critically different result than if not ignored." (Hammond, 1998, p. 4).

Deriving experimental stimuli by probing naturally occurring situations. If no such theory exists, researchers can follow Brunswik's approach to sample stimuli (or situations) by randomly probing naturally occurring situations. This can be done with experience or time sampling procedures that allow tapping into the everyday lives of (groups of) participants in a relatively unobtrusive way. This approach seems very promising when dealing with the ecologies of different age groups (e.g., Riediger & Freund, 2008). In this way, stimuli that are equivalent across age groups and representative of the ecologies of the different age groups can be identified.

Note, that not only stimuli might take on different meanings depending on the different ecologies that people experience in their lives, but the same applies to the responses to the stimuli. For instance, sucking intensity might be a good indicator of experiencing novelty in babies but is unlikely to be a reliable indicator of this construct in

school children. Again, the equivalence of these relations between the distal variables, the proximal or cue variables, and the response need to be established across age groups (see Freund & Isaacowitz, 2014, for an elaboration of the different kinds of validities in a Brunswikian framework and how they relate to developmental research). This is true not only for experiments but equally concerns correlative designs that involve self-report or behavioral observation. In the area of psychometric research on the development of personality and cognition, this is typically addressed by establishing measurement equivalence across age groups (e.g., Hertzog & Nesselroade, 2003). In contrast, only rarely is the issue of age-differential representativeness and validities made explicit in other areas of developmental research.

Even though arduous, I believe that developmental science needs to go this route of ensuring representativeness of stimulus materials and response options for different age groups. Otherwise, the omnipresent alternative interpretation of age-related differences is that the stimulus materials or response options provided by the experimenter takes on a different meaning for different age groups and because of this elicits age-differential responses instead of tapping into developmental processes causing these age-differences. Instead, researchers simply assume that holding the stimulus materials constant across different groups ensures comparability (in fact, most students might learn that this is one of the hallmarks of good experimentation). To avoid misunderstandings, the different meanings of the same stimuli in different age groups can also represent an interesting research topic, but typically it does not constitute the research question and is mostly just ignored.

Groups or individual persons? In this essay, I wrote mostly about age *groups* neglecting the interindividual variability in intraindividual change (or stability) over time. Although space restrictions do not allow elaborating on this very important and interesting issue, I would like to mention it at least in passing before concluding this essay. Group comparisons, be they between age groups or between experimental groups, can only give us very rough approximations of intraindividual processes and even might be misleading. It is easy to construct cases in which no individual shows the mean trajectory of their group. Therefore, approaches that fit models of change (or stability) to each individual person and, in a second step, group individuals based on the similarity of their trajectories will allow us to understand intraindividual developmental processes that might be camouflaged when only considering mean differences between (age or experimental) groups.

Conclusion. Taken together, I dream of a developmental science that is rooted strongly in precisely formulated theories that can be empirically tested and falsified. I wish this was done by rigorous empirical testing that makes use of experimental designs targeting—as directly as possible—the hypothesized developmental processes. To address the particular challenges of involving different age groups, a Brunswikian approach to experimentation implementing representative designs ensuring comparable validities across age groups seems like a dream come true.

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